clusion from the figures there given, except that this wire, either from its quality or its situation, behaved in a different manner from any one of the many specimens I have examined during the past five years. The value of $\delta R/\delta t$ decreases with increase of temperature in a most phenomenal manner.[1] In cases in which I have observed this phenomenon in a lesser degree it has indicated a breaking down in the insulation, consequent on rise in temperature. I am unable to find any evidence that the insulation was tested at high temperature during these experiments.

(4). Any expression of opinion by Messrs. Holborn and Wien necessarily carries weight; nevertheless, I would venture to suggest that (considering the amount of experimental work previously performed by those who advocate the methods of platinum thermometry) the examination of only two wires, one of which was admittedly exposed to the action of furnace gases, affords insufficient grounds for the adverse conclusions arrived at by the authors.

(5). Heycock and Neville's determinations of the freezing points of copper, gold, and silver are admitted by Holborn and Wien to be in "good agreement" with their own. The nature of this agreement is shown in the following table.

	Heycock and Neville.	Holborn and Wien.
Copper ...	1080·5	1082
Gold ...	1061·7	1072
Silver ...	960·7	971

It is worthy of notice that copper is the only metal of which Holborn and Wien used large quantities, comparable with the masses experimented on by Heycock and Neville. Also in this case Holborn and Wein determined both melting and freezing points. Their results (using practically the same thermometer throughout) range from 1076° to 1093°, whereas Heycock and Neville's values, when using six distinct platinum coils possessing very dissimilar constants, range from 1079°·0 to 1081°·7, a very different order of agreement.

In the case of gold and silver, Holborn and Wien used small quantities, determining their results by observations of the *melting* points, and thus the method of experiment adopted renders it probable that the temperatures observed would err on the side of excess. The close agreement in the case of copper, and the higher values found by Holborn and Wien in the experiments on gold and silver, are therefore significant.

Thus, when the conditions are similar (as in the case of copper), we may regard the results obtained by the different observers as practically identical. Such agreement would be impossible if platinum resistance thermometers ordinarily underwent, at high temperatures, changes of the nature of those observed in the wires studied by Holborn and Wien. The differences would then be measurable not by units, but by tens and hundreds! and these discrepancies would be found not only when different methods were used, but also when the same observations were repeated with different platinum thermometers.

If Tables XI., XII., and XIII. of Heycock and Neville's paper[2] are examined, it will be found that although in each case from 6 to 8 different platinum thermometers were used in which, for example, the value of δ varied from 1·495 to 2·04, the extreme resulting temperatures differ by a smaller quantity than the differences obtained by Holborn and Wien when repeating an observation without change in the conditions, by means of the same thermo-couple.

Finally, I assert that Holborn and Wien have produced no evidence sufficient to support the somewhat sweeping conclusions given by them on p. 394 of their paper. I have shown that in their experiments on only two samples of wire, they have neglected the precautions insisted upon by those who have devoted years of study and experiment to the investigation of the platinum thermometer, and this portion of their work is only useful in so far as it emphasises the validity of the conclusions arrived at by those who preceded them.

I fully appreciate the great value of Holborn and Wien's direct determinations of high temperatures by means of the thermo-couple and the air thermometer, and I admit it is probable that for temperatures exceeding 1400° C. or so, the thermo-couple is the more convenient, and possibly the more accurate instrument. Below such temperatures, however, I consider that the weight of evidence is in favour of the accuracy of the platinum thermometer. In any case, such evidence is in no way weakened by the experiments of Holborn and Wien.

E. H. GRIFFITHS.

Earth Tremors.

IN Prof. Milne's article in NATURE of December 26, he states that earth tremors are more frequent during the winter than during the summer, that they are frequent with a low barometer, and still more frequent when the locality of observation is crossed by steep barometrical gradients. In the North-West Himalayas, throughout the winter months, slight earth tremors are exceedingly frequent, and occur, so far as can be judged without instrumental records, more frequently by night than by day. This may be in part due to the fact that during the day most people would be moving about in downstairs rooms, while at night the same people would be in upstairs rooms, and both they and their surroundings perfectly quiet; but, whatever may be the day and night relation, there can be no doubt that during the winter months in Simla peculiar little earth tremors are remarkably frequent. My experience has been that these tremors are not so much connected with areas of low barometer as with the *commencement* of a sudden and large change in atmospheric pressure from a high to a low, a reduction of pressure which need not necessarily be accompanied with steep barometric gradients or high winds at or near the earth's surface. In the case of earthquakes, also, I have noticed subsequent large changes in atmospheric pressure. Thus at about midnight on January 15–16, 1896, a (for these regions) rather severe earthquake occurred, which lasted from 1m. 20s. to 4m. in different localities. On the plains the most severe shocks were felt at midnight, 15th, at Simla, at 0h. 30s. a.m. on the 16th, and at Srinagar at 1 a.m. on the 16th. Times for other places in the Punjab were published in the newspapers, but I have omitted to keep them. The above, however, show that the shock was felt at Lahore at midnight, at Simla half an hour later, and at Srinagar an hour later. The barometric records show that for the forty-eight hours from 8 a.m. on the 16th to 8 a.m. on the 18th, pressure changed as follows:—

	"
Srinagar (Kashmir)	−0·187
Astor ,, ...	−0·062
Murree	−0·165
Lahore	−0·144
Simla	−0·140
Quetta	+0·022

From the above figures, it appears that a considerable decrease of barometric pressure occurred between the morning of the 16th and the morning of the 18th, and that this fall was central over Srinagar; while the times of occurrence of the earthquake show that the movement of terrestrial disturbance was directed towards this central area of diminishing pressure. It has always appeared that the atmospheric changes which ordinarily occur in tropical and subtropical countries would be a wholly inadequate cause to account for the considerable earthquakes which at times occur; but I have undoubtedly noticed that very slight earth tremors constantly take place when a sudden and large decrease in atmospheric pressure *commences* after a considerable period of high pressure.

W. L. DALLAS.

Simla, January 29

"Roches moutonnées."

SOME ten years ago, I came across in an old memoir a rational explanation of the term *roches moutonnées;* but I made no note at the time, and have been unable to trace the reference. However, my scepticism was fortified, and I proceeded to search French dictionaries, which made it clear that *moutonné* meant "frizzled like sheep's wool," and not "sheep-like." Yet M de Lapparent tells us ("Traité de Géologie," 3me ed., p. 281) that these glaciated rocks "produisent une impression analogue à celle d'un troupeau de moutons endormis, d'où le nom de *roches moutonnées*"; and who shall question this precise statement of a French author interpreting his own language? It is the explanation that has been taught to all of us, though I know of only one field-geologist who seriously maintains that *roches moutonnées* might be taken for a flock of sheep. Agassiz states

---

[1] This extraordinary behaviour of Holborn and Wien's Sample II. (for reasons previously given the behaviour of Sample I. is of no significance) is noticeable in the numbers attained by them at *low*, as well as at high, emperatures.

[2] *Chem. Soc. Trans.*, 1895, pp. 188–190.

© 1896 Nature Publishing Group

that De Saussure is the author of the term; but I have long been foiled by its omission from the index of the famous "Voyages dans les Alpes." In "Open Air Studies," however, I ventured to compare the mammillations of a glaciated surface to those upon an antique wig; but all the time, it seems, Mr. Whymper held the key of the matter for us, in a passage which has escaped the memory even of Prof. Bonney (see "Ice-Work," 1896, p. 10). Mr. Whymper, in fact ("Scrambles amongst the Alps," fourth edition, 1893, p. 133), supplies the reference to De Saussure; and in the "Voyages dans les Alpes," 1804, tome ii. p. 435, par. 1061, we may read of what are styled in the margin "montagnes moutonnées." De Saussure states that behind Envionne (the modern Envionnaz), in the upper valley of the Rhône, "ces rondeurs contiguës et répetées forment en grand l'effet d'une toison bien fournie, ou de ces perruques que l'on nomme aussi *moutonnées*." In face of this, there is no longer any need to tax the credulity of our pupils with a fanciful explanation, which we seem to have forced even upon French-speaking peoples.

Grenville A. J. Cole.

Royal College of Science for Ireland,
Dublin, February 17.

The Age of the Present Canadian Flora.

Prof. D. P. Penhallow has recently identified some fragments of wood found in the Leda clays of Montreal, as *Picea nigra*, the common black spruce. This is another addition to the group of plants which represent our present knowledge of the flora of Canada in Pleistocene times. This Pleistocene flora may now be taken to include not merely the plants found in these Leda clays and in the clays believed to be equivalent to them in age in Ontario, but also the ancestors of the present inland maritime flora found on the shores of the Great Lakes, hundreds of miles from the sea-coast, and of the plants which are common to Europe and America, and which include so many arctic and sub-arctic, as well as northern temperate species. The inland maritime plants, and probably also the sub-arctic species now found so far south as the headlands of Lake Superior, made their way to their present localities during the deposit of the Leda clays when a considerable part of Eastern Canada was submerged. Six of the species which occur in the Leda clays at Ottawa and Montreal, and thirteen of the inland maritime plants, as well as several of the Lake Superior sub-arctic species, are also European, showing that at that period the intermingling of the American and European floras was well established, but leaving open the possibility of these plants common to the two continents being even older than the period of the Leda clays.

The intermingling of the Asiatic and American floras appears to have taken place at a still earlier period. The oldest known representatives of the existing Canadian flora are those few identical species found by Mr. Lester F. Ward in the Laramie rocks of the Western United States—rocks which Sir William Dawson refers to the Lower Eocene. Two of those identified now occur in both Japan and Canada; and one, still living in Japan, has, if correctly identified, become extinct on the American continent. Again, among the Leda clay and inland maritime plants there are several species which are likewise common to the two countries. The intermingling of Asiatic and American plants evidently took place prior to the upheaval of the Rocky Mountain chain, as the extensive flora peculiar to British Columbia, Oregon, and southward, is almost without a representative in Japan. This British Columbia flora, so well represented by various species of Claytonia, Lupinus, Trifolium, Astragalus, Saxifraga, &c., as well as Coniferæ, is of more recent birth—probably Later Tertiary and Post-Tertiary.

The most recent creations in Canada would appear to be the plants which—well represented by Compositæ, an order of no great antiquity—are now so marked a feature of the prairies of Manitoba and the surrounding country—prairies which in some places are still in process of formation.

A. T. Drummond.

Children's Drawings.

With regard to young children drawing upside down, I have for some time past collected observations. It is certainly true, that a great many children do draw in this way; on the other hand, many from the first draw the right way up. I have seen a boy of four, when asked to draw a rook on a haystack, begin at the bottom of the paper with the rook's back, and gradually work his way up to the haystack; he then turned it round, and handed it to me to look at, evidently realising that it was inverted.

I do not think the explanation depends in any way on the inversion of the retinal image. If a child, who draws upside down when drawing on a horizontal table, is asked to draw on a blackboard placed vertically, he will draw everything the right way upwards. It seems to me, that the explanation simply is that the child has to draw an object, which he has seen in a vertical plane, on paper placed in a horizontal plane—an extremely difficult task to him—and it is a mere question of convenience to him at which end he begins, both being equally wrong from his point of view. This will also explain why children sometimes look at picture-books upside down, and also why small children are much more ready to draw objects, which they have been accustomed to see in a horizontal plane, such as a plate with oranges on it, than an erect object.

The Old Palace, Richmond. Rina Scott.

THE RÖNTGEN RAYS.

The discovery by Prof. Röntgen of the rays which bear his name has aroused an interest perhaps unparalleled in the history of physical science. Reports of experiments on these rays come daily from laboratories in almost every part of the civilised world. A large part of these relate to the methods of producing Röntgen photographs, and the application of the "new photography" to medical and other purposes. A considerable amount of work has, however, been done on the physical properties of these rays; this has entirely confirmed the results stated by Röntgen in the paper in which he announced his discovery. The freedom of refraction of these waves, in which they are different from ordinary light, has been the subject of direct experiments made by M. Perrin and by Dr. Joly, while Dr. Lodge and others have confirmed the absence of any deflection in the magnetic field which differentiates these rays from the ordinary kathode rays.

Up to the present, however, no phenomena have been observed which enable us to say whether these waves are or are not transverse vibrations of very small wave-length, longitudinal vibrations, or even vibrations at all. Nothing of the nature of polarisation or of interference has been described. The absence of polarisation can at the present stage of the investigation hardly be pressed as an argument against these rays being transverse vibrations. For, of the three methods of producing polarisation in light —reflection, refraction, and absorption—only the latter is available for these rays. Now the number of substances which produce sensible polarisation in ordinary light by absorption is very small, and unless a much larger number possess this property for the Röntgen rays, it is hardly likely that, even if there are such substances, they would have been discovered in the three months which have elapsed since the publication of Röntgen's discovery. I may remark that I have made a large number of experiments on the opacity to these rays of plates of tourmaline (1) with their axes crossed, (2) with their axes parallel, testing the intensity of the rays which came through in some cases by their action on a photographic plate, in others by the discharge they produced in an electrified plate on which they were incident. The result of these experiments was entirely negative, for although the tourmaline plates produced very considerable absorption of the rays, no difference was detected between the absorption when the axes were crossed and when they were parallel. It is very desirable that a large number of substances should be tested in this way.

M. C. Henri has made the very interesting observation that an opaque coin coated with the phosphorescent sulphide of zinc will allow these rays to pass through it; the details of this experiment will be received with much

© 1896 Nature Publishing Group